These five are all "representative" dreams; and with the possible exception of two, as "hallucinatory" as dreams can well be. If the evidence be still regarded as unconvincing, I would propose that trial be made of autosuggestion. This can, of course, be done without any lapse into that form of the "psychologist's fallacy," against which Professor Murray cautions dream observers.[1]

Postscript. Since the above paper was sent in to the Editor, Sept. 14, I have received accounts of three more taste dreams; two from new observers. No one of them was suggested. Since that date, also, there has appeared Professor Ribot's article, *Recherches sur la Mémoire affective* (*Rev. phil.*, Oct., 1894), which confirms many of my arguments.

VII.

ON THE QUANTITATIVE DETERMINATION OF AN OPTICAL ILLUSION.

(Continued.)

BY R. WATANABE, PH. D.

On page 418 of the current volume of this JOURNAL, Mr. Knox writes, apropos of the dotted-line and point-distance illusion, as follows: "Binocular bisection of horizontal distances is not subject to any constant error; binocular bisection of verticals is subject to the constant error of overestimation of the upper part of the field of vision. We should, therefore, expect to find the *m. v.* of our vertical △'s greater than that of our horizontal. The results [do not verify this expectation] . . . . This is curious. We are unable to offer any explanation of the result."

Further experiments upon the illusion in question were made, in the hope of elucidating this difficulty. Every precaution that could be thought of was taken to ensure accuracy and avoid the intrusion of complicating factors. Mr. Knox' experiments were exactly repeated, with a single modification. Whereas, on his cards, the point-distance was constant, and the dotted-line variable, on our own the reverse was the case. We imagined that if this alteration in the nature of stimulus brought about any alteration in judgment, the latter would be of such a kind as to be readily determinable for itself; and that this determination, itself an interesting side issue, would not interfere with the realization of the main object of the new experiments. On the other hand, the stimulus altera-

[1] P. 21.

tion might prove to be without influence upon the judgment process; in which case we should at least obtain the negative result.

Series were obtained from three subjects: Messrs. Knox (*K.*; see pp. 416, 419), Pillsbury (*P.*; see pp. 417, 419, 421), and Read (*R.*). The following tables correspond in every respect to those of pp. 415 ff.:

TABLE I.

Reagent *K.* Vision normal. Method *(e)* predominant. General and special practice. Unit = 1 mm.

SERIES.	*R.*											
	C=25	*n.*	*m.v.*	*C=30*	*n.*	*m.v.*	*C=35*	*n.*	*m.v.*	*C=40*	*n.*	*m.v.*
C—V	27.18	3	0.62	32.12	3	0.62	36.62	3	0.78	42.62	3	1.03
V—C	25.25	3	1.66	32.00	3	1.25	36.95	3	0.80	42.00	3	0.91
$\frac{V}{C}$	26.29	3	0.82	31.95	3	0.63	37.16	3	0.63	42.16	3	1.08
$\frac{V}{C}$	26.71	3	0.90	31.37	3	0.73	36.95	3	0.64	41.74	3	0.79
Hor. △	+1.21			+2.12			+1.78			+2.31		
Vert. △	+1.50			+1.66			+2.05			+1.95		

TABLE II.

Reagent *P.* Vision Normal. Method *(b)* predominant. General and special practice. Unit = 1 mm.

SERIES.	*R.*											
	C=25	*n.*	*m.v.*	*C=30*	*n.*	*m.v.*	*C=35*	*n.*	*m.v.*	*C=40*	*n.*	*m.v.*
C—V	25.08	12	0.81	30.89	11	0.56	35.78	12	0.66	40.70	12	0.79
V—C	25.40	12	0.69	31.65	12	0.58	36.58	12	0.80	41.92	12	1.06
$\frac{V}{C}$	26.48	9	0.57	32.84	9	0.65	37.81	9	0.72	43.60	9	0.91
$\frac{C}{V}$	26.44	9	0.86	32.40	9	0.68	37.37	9	0.85	42.61	9	0.83
Hor. △	+0.24			+1.27			+1.18			+1.31		
Vert. △	+1.46			+2.62			+2.59			+3.10		

TABLE III.

Reagent *R.* Vision normal. Special practice only. Method mixed. Unit = 1 mm.

SERIES.	*R.*											
	C=25	*n.*	*m.v.*	*C*=30	*n.*	*m.v.*	*C*=35	*n.*	*m.v.*	*C*=40	*n.*	*m.v.*
C—V	27.10	6	1.27	31.39	6	1.60	37.26	6	1.39	41.89	6	1.17
V—C	27.49	6	1.08	32.83	6	1.32	37.53	6	1.48	42.49	6	1.41
$\frac{V}{C}$	27.37	6	1.27	32.29	6	1.51	37.66	6	1.57	42.41	6	1.47
$\frac{C}{V}$	26.72	6	1.37	31.25	6	1.33	36.83	6	1.22	41.74	6	1.33
Hor. △	+2.29			+2.11			+2.39			+2.19		
Vert. △	+2.04			+1.77			+2.24			+2.07		

Remarks.[1] (1) The illusion holds for every observer.

(2) Vertical △'s are larger than horizontal, in these twelve comparisons, in six cases; smaller in six. But in none of these contrary cases does the difference of the two △'s amount to half a mm. (Differences are: Table I—0.46, 0.36; Table III—0.25, 0.34, 0.15, 0.12.) Moreover, four of them come from the least practised reagent, *R.*, who began with horizontal judgments. Mr. Knox' conclusion under this head is, therefore, confirmed by our results.

(3) The main object of the present investigation has been stated above. Do we find any light thrown upon the matter by the present figures? We have:

Table I. *r* 0, = 2 (.79, .63; .97, .93), *w* 2 (1.72, 0.86; .93, .68).

Table II. *r* 0, = 4 (.75, .71; .57, .66; .73, .78; .92, .87), *w* 0.

Table III. *r* 2 (1.17, 1.32; 1.29, 1.40), =2 (1.46, 1.42; 1.43, 1.39), *w* 0.

In all, *r* 2, *w* 2, = 8. Mr. Knox obtained *r* 6, *w* 7, =7; or, if his Table I be included, *r* 8, *w* 8, = 8. Massing, therefore, we get *r* 10, *w* 10, = 16. We do not insist upon the absolute relations of these figures,—apart from the fact that massing, even in two such comparable cases as these, is psychologically unjustifiable. Nor do we fail to note that of

[1] See pp. 418 ff.

our own = 8, six have what tendency to differ they do have in the direction of w; of Mr. Knox' = 8, seven have such a tendency. But we feel safe in formulating the following proposition: *so far as Mr. Knox' and our own experiments extend, there is strong evidence that, in presence of the dotted-line and point-distance illusion, the illusion of over-estimation of the upper half of the field of vision disappears;* the evidence being couched in terms of the *m. v.* in vertical and horizontal quantitative determinations of the former illusion. We are not at present prepared to suggest any explanation of this fact. The fact itself holds, whether we employ the method with knowledge or the method without knowledge, and whether the reagent be practised or comparatively unpractised in *Augenmass* experiments.

(4) The values of $\frac{\triangle}{r}$ are:

I. *Hor.*: $\frac{1}{20}$, $\frac{1}{14}$, $\frac{1}{19-20}$, $\frac{1}{17}$. *Vert.*: $\frac{1}{17}$, $\frac{1}{18}$, $\frac{1}{17}$, $\frac{1}{20-21}$.

II. *Hor.*: $\frac{1}{104}$, $\frac{1}{23-24}$, $\frac{1}{29-30}$, $\frac{1}{30-31}$. *Vert.*: $\frac{1}{17}$, $\frac{1}{11-12}$, $\frac{1}{13-14}$, $\frac{1}{13}$.

III. *Hor.*: $\frac{1}{11}$, $\frac{1}{14}$, $\frac{1}{14-15}$, $\frac{1}{19-20}$. *Vert.*: $\frac{1}{12}$, $\frac{1}{16-17}$, $\frac{1}{15-16}$, $\frac{1}{19-20}$.

(a) Table I is taken from the same reagent as Mr. Knox' Table III. The values of its limina are probably vitiated for the reason alleged on page 419. *(b)* Table II is taken from the over-practised reagent *P.* (See page 419.) Here, as in Mr. Knox' Table IV, the vertical $\triangle$'s have suffered much less by practice than have the horizontal. *(c)* Table III, from a previously unpractised reagent, confirms Mr. Knox' general formula (p. 419) a good deal better than his own Table I, from a similar reagent, does. *(d)* The valuelessness of $C = 40$ mm. is indicated by Table III. We should not expect to find evidence of it in the other two tables. It is, perhaps, hardly necessary to make the explicit statement that these supplementary experiments were not at all expected to throw light on the magnitude of $\frac{\triangle}{r}$. The two reagents *K.* and *P.* were wholly unsuitable for such a purpose. On the other hand, the reasons that disqualify them for that investigation do not come into account for the main issue, discussed under (3). And it is, at least, satisfactory to note that there is nothing in the fractions which makes against Mr. Knox' conclusions. Thus, Tables I and II alike make the vertical $\frac{\triangle}{r}$ greater, on the average, than the horizontal; and the variation from this rule, in Table III, is so slight as

to be readily explicable in terms of the order of special practice (*cf.* Table I, p. 419). *(e)* Did the reversal of the *C* and *V*, as compared with those of Mr. Knox' investigation, influence the process of judgment? (i) The opinion of the reagents *K.* and *P.* was to the effect that it did not. (ii) If we compare the fractions obtained by the old and new methods, we find that those of the latter case are:

Table I. *Hor.*: $< < < >$; *Vert.*: $< < > <$,

Table II. *Hor.*: much $< = < <$; *Vert.*: $<$ just $< = =$, as compared with those of the former. This general lessening was to have been expected, other things equal, from the increase of practice. Had the interchange of *C* and *V* had any influence, it would, we think, have been one in opposition to this tendency to lessen. For *a priori*, if there is any question of relative ease or difficulty, it should be easier to estimate when the dotted line varies (Mr. Knox' procedure) than when the point distance is the variable (our own). In the former case, an extension difference carries with it a quality difference, a greater or less number of dots: while, when the extension of the point-distance alters, no qualitative change is involved. The fact that the practice-lessening of the fractions is so little counteracted, therefore, in our results, tends to confirm the verdict of introspection. (iii) Moreover, the horizontal figures of Table III show, as has been pointed out, a very good agreement with Mr. Knox' formula. On the whole, then, we would answer the question of this paragraph in the negative. *(f)* Since the appearance of Mr. Knox' paper, there has been published in the *Zeitschr. f. Psych. u. Physiol. d. Sinnesorg.*, an attempt at a quantitative treatment of another optical illusion—that of the arrow head and feather *(F. Auerbach: Erklärung der Brentanoschen optischen Taüschung*, Vol. VII, pp. 152 ff.). The numerical results (p. 159) are not comparable with those given in the two present papers; the point investigated being not the quantitative variation of the illusion with variation of absolute magnitude of lines or distances, but its increase with increasing length of the limbs of the limiting right angles. But attention may be called to certain remarks of the writer's, bearing upon the general question. (i) The illusion varies with the *visual habits* of the reagent (p. 155). We have had no opportunity of testing this, in Auerbach's way. But the statement receives indirect confirmation from Mr. Knox' conclusions, p. 419 of this JOURNAL. (ii) Increased concentration of vision and attention diminishes the illusion (p. 155). This holds of the arrow head and feather illusion, for the explanation of which the influence of indirect vision is called into account, to a greater extent than for our own.

At the same time we have seen that a similar result may be obtained by familiarity and practice. (iii) It is necessary, for determination of the limen of difference, to avoid knowledge of the actual relations of the distances compared on the part of the reagent (p. 159). This point has also been insisted on by Mr. Knox. (iv) Judgment should be as immediate as possible; since it is apt to fluctuate, if the stimulus is present for any length of time (p. 159). *Cf.* the length of Mr. Knox' experimental series (p. 414). The method employed both by Auerbach and ourselves being a form of minimal changes, the necessity of immediacy of judgment is a matter of course.

VIII.

The Cutaneous Estimation of Open and Filled Space.

By Professor C. S. Parrish.

A comparison of the spatial functioning of the cutaneous and visual sensibilities must always possess an especial psychophysical interest. The study of sensational intensities culminates in Weber's Law; that of sensational quality leads to a whole number of alternative psychophysical theories; the determination of the temporal attributes of sensation is one means of approaching the problems of the so-called time-sense; that of its spatial attributes, the first step towards a psychological space construction. But, whereas every sensation is possessed of duration, quality, and (with the exception of the visual series) intensity, a space attribute attaches exclusively to the sensations of sight and pressure. This fact, which seems at first sight to simplify the space problem, in reality renders that problem unusually difficult of solution.

Opinions differ very widely as regards the sensational factor in psychological space, as regards the interaction of eye and skin in its construction, and as regards the attributes and aspects of the cutaneous sensibility itself. It may, therefore, be well to give here, at the outset, a brief *credo*, not with any intention of dogmatizing, but merely with a view to clearness and intelligibility.

We believe, then, that the development of the eye, as a space organ, far outran that of the skin. That tactual space was, accordingly, built up under the influence of, and remains almost invariably subject to that of vision. Nevertheless, that there are two psychological spaces, and not one space. We consider, further, that the mechanical cutaneous sensi-